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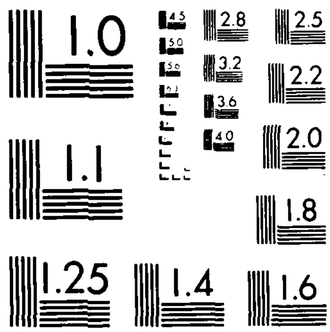

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Generality of the Effects of Practice

on Social Judgment Tasks

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Generality of the Effects of Practice on Social Judgment Tasks

Abstract

Many types of social and nonsocial cognitive processes can be performed faster after they have been practiced. Two experiments examine some properties of this speedup, particularly its pattern of generality, in order to give indications as to its underlying theoretical basis. For example, consider a person who practices judging whether a number of behaviors imply a particular target trait. Is the resulting increase in speed specific to the behaviors that were judged, does it apply to judgments of new behaviors with respect to the same target trait, or is it applicable to all judgments using the same process, even for different target traits? These experiments identify components of speedup that show each of these patterns. The results show that general procedures can be strengthened by practice and contradict the notion that speedup is purely a function of increased accessibility of schemas or other memory structures representing knowledge about the judgment target. The effects of practice need not be content-specific.

Generality of the Effects of Practice on Social Judgment Tasks

People get better at activities when they practice. This truism holds for an extremely wide range of cognitive and motor skills (Newell & Rosenbloom, 1981). Often the focus of interest is the time taken to perform a task, which is generally found to decrease as a power function of the number of trials of practice with the task. Social judgments follow this general rule. The efficiency with which people can make social judgments, as measured by the speed and accuracy of performance, increases systematically with practice (Smith & Lerner, 1986).

Increased efficiency has implications beyond the obvious ones for response times and error rates. Fazio (1986) has shown that increases in the speed with which perceivers can retrieve their attitude toward social objects (induced by practice in the form of repeated attitude expression) has important consequences, increasing both attitude-behavior consistency and resistance to persuasion. As another example, the efficiency of a judgment process may influence the likelihood that a perceiver will make a particular judgment in the course of interacting with others or comprehending social information, and therefore may influence the content of an impression or the outcome of interaction. For example, if observer A is particularly fast and efficient at making judgments concerning the trait of honesty, while observer B is adept at making judgments concerning friendliness, they might form substantially different impressions of another person based on a common set of behaviors. This might be particularly true if some of the behaviors

were honest but unfriendly (like giving a frank negative opinion of someone's taste in art) or dishonest but friendly (like sharing answers with someone on an exam).

At issue in this paper are (a) the identification of some properties of the increase of judgment speed with practice, particularly the degree of specificity or generality of the effects of practice. From these properties, (b) certain theoretical conclusions may be drawn regarding the nature of the underlying changes that are brought about by practice. The paper will begin by describing some of the background and prior research relevant to these two issues, then present two experiments. Finally, the theoretical implications of the findings will be discussed in some depth.

Properties of Practice Effects

Experiments reported by Smith and Lerner (1986) demonstrated effects of practice on a range of social judgments similar to those that have been used in studies of social cognition. The tasks included judging the likability of a target character described by a list of traits, judging the self-descriptiveness of a similar trait list, judging whether a set of political positions could be described as "liberal," and verifying the gender of common first names. Those experiments showed (a) social judgments speed up with practice by a power law, and major increases in speed are evident with just 30-100 trials of practice. (b) The speed-up is accompanied by a decrease in errors, pointing to an underlying increase in efficiency rather than a change in speed-accuracy trade-off. (c) The effects of practice are

still evident on the speed of judgments made 24 hours later. (d) The effects of practice have some generality. For example, when subjects practiced verifying whether or not trait lists matched a particular target stereotype (say of a waitress), that practice greatly increased their speed on the task of verifying a <u>different</u> stereotype (say a librarian). Whatever cognitive processes or procedures are made more efficient by practice are not narrowly specific to the content of the judgment being made.

This issue of generality is crucial, because research in different paradigms obtains conflicting results. For clarity in describing tasks in this paper, we will use the following terms. <u>Stimuli</u> or <u>items</u> refer to words or other objects, usually differing on each trial, that appear on a screen and to which subjects respond. A <u>target</u> is held in memory, defining the subject's search or judgment task, and is usually constant across many trials. Shiffrin and Schneider (1977) studied the effects of extended practice on visual search and detection tasks. In a typical task, on each trial subjects examine an array of letters presented on a screen and press a key to signal whether or not any of the stimuli match any of a predefined set of target letters. In this paradigm, the effects of practice are specific to the practiced targets and distractors. Practice with one set of target letters does not help subjects search for new targets; in fact, performance deteriorates to below baseline if items previously practiced as targets are used as distractors. The theoretical basis for the speedup (cf. Shiffrin & Dumais, 1981) is the development of an automatic attentional response to consistently practiced targets and also a diminution of responses to

items practiced as distractors. Thus, the effects of practice are content-specific.

Automatic attentional responses in search tasks need not be specific to the actual physical displays subjects examine. In a study of category search by Fisk and Schneider (1983), subjects searched for words belonging to a common category like furniture or animals. Extensive practice increased their efficiency on the set of category members on which they had practiced, as expected. But they also showed some increased efficiency in searching for unpracticed members of the target category. Their learning of automatic attentional responses went beyond the studied category members, but was specific to the studied category.

These studies involve search tasks, which differ in many ways from the social judgments that are of concern here. They also use considerably more trials of practice than our experiments, mostly because of the adoption of a particular operational definition of "automaticity" as a criterion to which subjects are brought. These investigators, influenced by Shiffrin and Schneider's (1977) theoretical position, believe that search tasks make a transition from serial (element-by-element comparisons) to parallel under the impact of extensive practice, and that the transition can be detected by a diminution (ideally to zero) of the slope relating response time (RT) to the number of targets being searched for. A parallel search does not take any longer to search for any of four targets than to search for a single target. Normally it takes many hours of practice for subjects to reach this zero-slope criterion with search tasks.

Recently it has been argued that the vanishing of the slope may be artifactual, a function of subjects' strategies for performing the task (Logan & Stadler, 1986). In support of this idea, Logan and Stadler showed that a zero slope could be obtained within a single session under appropriate conditions and that subjects made errors on as many as 50% of the trials where the strategy they hypothesized would lead to erroneous responses. Therefore, the use of the zero-slope criterion may be inappropriate. Our own preference is to avoid theoretical controversies concerning the nature of "automatic" processing and its many proposed operational definitions. We focus instead on the criterion of processing efficiency (measured by RT). In turn, this criterion leads us to use relatively small amounts of practice (up to a few hundred trials), because almost all of the power-law decrease of RT takes place early in the series of trials. With the tasks we have studied, the power law predicts that further decreases in RT after a few hundred trials would be small.

These experiments, then, will continue the investigation, begun by Smith and Lerner (1986), of the properties of practice effects on social judgments. The experiments will use different types of social judgments to confirm the wide applicability of the power-law practice speedup. They will examine how much consistency of practice is necessary for the development of increased efficiency. And, most important, they will look at the degree of generality of the increases in efficiency produced by practice.

Theoretical Accounts of Practice Effects

The properties of practice effects are of considerable interest in

their own right, but they are also of substantial theoretical significance. There are at least three competing models of how practice increases the efficiency of performance, which generate different predictions, particularly for the generality of practice effects.

The most prominent theory is that of Shiffrin and Schneider (1977), already mentioned. This theory is largely based on studies of automatic attentional responses developed by practice with search and detection tasks, and predicts that practice effects are specific to practiced stimuli (whether at the level of the items or the category; Fisk & Schneider, 1983). Items that were practiced as search targets automatically elicit attentional responses, and items that were practiced as distractors lose some of their ability to draw attention. Therefore, some positive transfer is observed when new targets are introduced among practiced distractors or new distractors are introduced as a background for practiced targets (Shiffrin & Dumais, 1981). However, the underlying changes induced by practice, shifts in the ability of particular items to draw attention, are item-specific. Negative transfer is observed if previously practiced distractors are made targets or vice versa.

Ideas related to the Shiffrin and Schneider conception of content-specific increases in efficiency have appeared within social cognition. We call these ideas "related" rather than direct derivations from the Shiffrin and Schneider theory because there are many differences between search and detection tasks and the kinds of judgments of concern to social psychologists. However, Schneider, Dumais, & Shiffrin, 1984, "think it likely that [their] conclusions should generalize to the

automatization of other processes as well" (p. 18). For example, Fong and Markus (1982) link the notion of having a self-schema on a particular dimension (such as extraversion) to being an "expert" on that dimension, implying a high level of efficiency and accuracy in judgments along that dimension. They demonstrate that schematics' judgments about other people differ from those of nonschematics in ways that would be predicted by this hypothesis. The expertise is specific to the target (schematic) trait rather than constituting a general expertise in making all kinds of trait judgments.

A second theoretical perspective is Anderson's ACT* (1983), which Smith (1984) has advocated as a general framework for theories in social cognition. ACT* can account for increases in efficiency with practice through several mechanisms. Cognitive processes make an initial transition from slow but flexible "interpretive" mode of application by being compiled into cognitive procedures when a process is executed a few times, producing a large initial speedup. Thereafter, the speedup of RT is dominated by two mechanisms, the strengthening of procedures and of data stored in memory (Anderson, 1982). Specifically, RT is *inversely proportional to the product of procedure strength and data strength* (Anderson, 1983, p. 151). Compilation and procedural strengthening can yield practice effects that are not target-specific, because the strengthened procedures will operate more quickly even with new (unpracticed) targets. For example, general procedures for searching would be strengthened by practice searching for target A, and this would facilitate later searches for a different target B. Data strengthening will generate target-specific practice effects, like those

predicted by Shiffrin and Schneider, because only operations that reference the same target will benefit from the ability of data that has been activated in memory to be retrieved more quickly.

Logan (1986) has recently proposed an instance theory of automatization. Both of the above theories hold that a process is performed in basically the same way as its efficiency increases with practice.[1] Instead, Logan holds that practice enables people to switch to a basically different mechanism. When performing a novel task, people use some process or algorithm to analyze the stimulus information and give an appropriate response. This takes a certain amount of time. Later, people may encounter a particular stimulus that they have seen before. This provides a second way to come up with the response: either the algorithm or memory retrieval can be used. People are assumed to store information about each item they encounter and the response that they make, so when an item is repeated, it may be possible to retrieve the response from memory more quickly than to recompute it. Lingle and Ostrom (1979) and others have also discussed the retrieval of previously stored social judgments, but Logan's (1986) contribution is to demonstrate quantitatively that under some circumstances this notion predicts the empirically observed rates of speedup with practice. Speedup from this memory retrieval mechanism must be specific, not only to practiced targets, but to old, previously encountered stimulus items. This contrasts with the Shiffrin and Schneider theory, which makes no special predictions concerning repeated stimuli (only regarding the consistency of search targets).

In Logan's theory there is no change in the algorithm or in its speed as practice proceeds, only a reduction in the probability of using

the algorithm and an increase in the probability of using a faster memory retrieval process instead. Of course, both an increase in processing efficiency and the use of memory retrieval for old items could occur; Logan's theory in its current version assumes that the former does not take place, but is not inconsistent with it.

Therefore, examining the effects of practice on performance with unpracticed items and targets can help distinguish among these theories. If social judgment follows the principles discussed by Shiffrin and Schneider with respect to automatic attentional responses, we should observe a speedup for practiced targets that will not transfer to new targets. Anderson's model predicts a speedup when a practiced process is applied to unpracticed targets, due to the strengthening of general procedures. It also accommodates (via data strengthening) target-specific speedup. Logan's theory alone predicts facilitation for repeated stimulus items (when the target is held constant).

Experiment 1

In both of the experiments in this paper, subjects judged whether a particular behavior implies a particular trait. This type of judgment is important in everyday social perception, for we frequently learn about other people's traits by observing and making inferences from their overt behaviors. In addition, this sort of task has been extensively used in research on social cognition, for example in studies of category accessibility (e.g., Srull & Wyer, 1979) where "priming" manipulations are found to influence the trait inferences subjects make from specific behavioral descriptions.

In experiment 1, subjects practiced various combinations of such judgments. The primary interest is in two conditions: repeatedly judging a single target behavior with respect to different traits (same behavior-different traits or SBDT), and judging different behaviors with respect to a single target trait (different behaviors-same trait, DBST). Experiment 1 also used two control conditions: different behaviors-different traits (DBDT), where both the behavior and the trait differed on each trial, and same behavior-same trait (SBST), where both were repeated identically from trial to trial. The DBDT condition should not lead to the development of increased efficiency, as previous studies have shown that some measure of consistency of processing is essential (Schneider & Shiffrin, 1977; Smith & Lerner, 1986). This condition will therefore control for extraneous sources of speedup, caused (for instance) by subjects' learning where on the screen the question will appear and learning to press the response buttons more rapidly. The SBST condition should reveal the maximum speedup that is possible when all components of processing (the exact wording of the question and the response) are held constant. The comparisons of interest are the speedup in the SBDT and DBST conditions, relative to SBST.

Design

In Experiment 1, each subject made 60 yes/no judgments of the form "*If I <behavior> you, am I <trait>?*" This question, with specific behavior and trait words filled in, appeared on the screen on each trial to continually remind subjects of the nature of their task. Examples are: "If I hurt you, am I sincere?" "If I flatter you, am I unfriendly?" Subjects responded by pressing a yes or no key on a keyboard, under speed and accuracy instructions.

The experiment was a 2x2x2 design with one cell missing. The first two factors are same behavior (insult) versus different behaviors on each trial, and same trait (unfriendly) versus different traits on each trial. The varying behaviors and traits were randomly picked with replacement for each trial from lists of 30 behavior or trait words respectively. The third factor was embedded versus nonembedded. In the embedded condition, only 30 of the trials were actually experimental trials based on the same/different design. These appeared interspersed among the subject's 60 trials. The other 30 dummy trials were questions involving the *same* type of judgment but with unrelated and nonrepeated behaviors and traits. In the nonembedded condition, all 60 trials were experimental trials. The same behavior/same trait/nonembedded condition was not used. The primary dependent measure is speed on each trial.

We cannot assess error rate in these studies because, as with most social information processing tasks, the judgment is relatively subjective, and correct answers cannot be determined a priori. Therefore it is conceivable that subjects alter their speed-accuracy trade-off as the study progresses, making less accurate responses to increase their speed. However, we do not believe that this possibility affects the results of these experiments. In general, subjects probably assume that we as researchers will be evaluating their mental health or the normality of their social judgments based on their responses, so they are probably fearful of making unnecessary errors. There are two more specific arguments against this possibility. (a) Some tasks in the Smith and Lerner (1986) studies did have objectively correct responses, and for these tasks error rates actually decreased with practice as did

RTs. (b) The most difficult condition in this experiment (as measured by overall RT and by subjects' reports) is clearly DBDT, so if subjects were going to save time and effort by responding sloppily they would have done so in that condition if anywhere. Yet that condition showed no speedup. It is difficult to imagine why subjects would alter their speed-accuracy trade-off to save time in less difficult conditions but not in the DBDT condition.

Subjects and Procedure

Subjects were 83 undergraduates who participated as part of a course requirement. They were tested individually and responded to displays on a computer screen by pressing a yes or no key on each trial, under speed and accuracy instructions. There was only a brief delay (approximately 750 msec) between trials. After receiving verbal instructions, subjects completed five practice trials to make sure that they understood the procedure, then the 60 experimental trials. Finally subjects were debriefed and dismissed.

Results

The trial-by-trial speedup was analyzed by fitting a power function to the observations averaged across all subjects in a particular condition. The power function

$$RT = a \cdot n^b$$

where n is the trial number (1-60) and *a* and *b* are parameters, was fit by logarithmically transforming both the RT and trial number and applying least squares linear regression. The fit did not use an asymptote term, because (as in Smith & Lerner, 1986) the limited number of trials used did not permit the estimation of the asymptotic RT. The

speedup coefficient *b* indicates the rate at which performance improves with practice. Figure 1 shows a log-log plot of condition SBDT-nonembedded (trial-by-trial average times across the 12 subjects in that condition) and illustrates that the power function, a straight line in that space, fits the obtained data reasonably well ($R^2 = .44$, MSE = .13 log units). Table 1 presents the coefficients.

Figure 1 and Table 1 about here.

As the table shows, when only experimental trials are present, subjects could speed up when either the same behavior or the same trait is referenced repeatedly (DBST and SBDT conditions). The speedup was greater for the same trait than for the same behavior ($t(114)$ comparing the speedup coefficients = 2.92, $p < .01$). Almost none of the speedup observed in these conditions was due to general perceptual or motor factors such as learning the screen location of the question or pressing the response buttons. This conclusion follows from the observation that as predicted, no speedup appeared in the DBDT conditions, which would benefit equally from any such factors.

The SBST-embedded condition speeded up greatly (see Table 1), showing that subjects were able to take advantage of the repetition of the entire question intermixed with dummy trials. However, though both the DBST and SBDT conditions speeded up significantly under nonembedded conditions, they did not when they were intermixed with an equal number of dummy trials with varying contents. The failure to detect a speedup is not simply a function of the reduced number of experimental trials

(30 instead of 60) in the embedded condition. The speedup in the DBST and SBDT nonembedded conditions was significant even when only the first 30 trials are examined (coefficients are -.165*** and -.101*** respectively), so 30 trials suffice to detect a speedup if it is present.

However, a different way of looking at the DBST embedded condition (Figure 2) shows that the experimental trials did speed up over time, at least relative to the dummy trials. The experimental trials speeded up slightly (nonsignificantly) while the dummy trials slowed down. The combination of these trends means that by the last third of the trials (41-60) the mean time for experimental trials (2857 msec) was significantly less than for dummy trials (3632 msec; correlated $t(12)$ for subjects in this condition = 3.14, $p < .01$). (No such difference occurred in the SBDT-embedded condition.) Subjects were able to gain some efficiency on the experimental questions concerning the same trait even though these trials were intermixed with varying dummy trials.

Figure 2 about here.

Discussion

Experiment 1 produced several notable results. These behavior-trait judgments speed up with practice, as do the different types of judgments studied by Smith and Lerner (1986). The speedup is not due to peripheral factors such as learning where the items appear on the screen and learning how to press the response buttons faster. Unless the judgment is identical on each trial, embedding the experimental trials

in an equal number of varying dummy trials appears to greatly diminish the speedup rate, though there is evidence of some speedup on experimental trials in the DBST-embedded condition.

Experiment 1 leaves two major questions. (a) There is an evident asymmetry in the results, in that focusing on the target trait (DBST condition) gives a greater speedup than focusing on the target behavior (SBDT condition). Before we look at possible implications of this asymmetry, however, some possible artifactual explanations must be considered. First, only one target trait (<u>unfriendly</u>) and one target behavior (<u>insult</u>) were used, and these particular words might have unique properties. Second, the trait and behavior appeared in different positions in the sentence on the screen. If subjects read the whole sentence on each trial from the beginning up to the point at which the variable word appeared, they would take more time to do so when the trait (at the end of the sentence) varied than when the behavior varied. Though this notion would predict a constant time difference favoring the DBST over the SBDT condition rather than a difference in the rate of speedup, something about the position of the words in the display might somehow have influenced the speedup results. So the first question is, is the apparent asymmetry real, and if so what does it imply?

(b) Experiment 1 is unable to show how much of the speedup is general and how much is due to item-specific practice. Recall that the varying traits or behaviors were drawn randomly for each trial from a pool of 30 items. Thus, as subjects progress through 60 trials, the probability that the current item had been seen before increases steadily from 0 at the first trial to 1/30 or .033 at the second trial

to .865 at the 60th trial. (*In general the probability is* $1 - (29/30)^{(i-1)}$ at the ith trial.) One possible model is that all learning is item-specific (cf. Logan, 1986). Assume that subjects take a long time to respond to any new item and a relatively short time to respond to any repeated item, with both of these times being constant across trials. Then the growing probability of encountering a repeated item later in the sequence of trials would lead to a speedup. This speedup would not follow the power law, for it would give a linear relationship between RT and log(trials) rather than between log(RT) and log(trials).[2] However, the deviations might be undetectable in the face of random variability. The speedup observed in Experiment 1 might therefore depend on item-specific learning or on increases in the efficiency of general procedures (or both). The resolution of this uncertainty is the second major question that will be addressed by Experiment 2.

Experiment 2

Experiment 2, which repeats only the DBST and SBDT conditions from Experiment 1, has three main purposes. First, it is intended to evaluate the generality of the asymmetric speedup that Experiment 1 found in those two conditions. Experiment 2 uses an identical screen display (a single trait or behavior word) in both conditions, instead of the several-word question used in Experiment 1. Experiment 2 also uses several different target traits and behaviors instead of the single, perhaps unique ones used in Experiment 1. These changes should allow an assessment of the possibility that the asymmetry found in Experiment 1 might have been due to theoretically uninteresting factors.

Second, Experiment 2 is intended to assess the effects of item repetition on speedup. Subjects completed five blocks of behavior-trait judgments like those of Experiment 1, with 50 judgments per block. Within each block some items were repeated, at varying intervals after their initial presentation. We will assess the speed of response to repeated items compared to new items, and see whether new, nonrepeated items speed up at all.

Third, Experiment 2 will determine the relative impact of target-specific and general learning. In the final block all subjects were transferred to a new judgment target. Their performance will indicate how much of the learning from the prior four blocks of practice was target-specific and how much was general.

Design and Procedure

Experiment 2 has only two conditions, DBST and SBDT, with 12 subjects per condition. Subjects participated for experimental credit and were tested individually. They were told that we were interested in judgments of the form: "is a particular action that a person might do to another person (like HITTING) a particular type of action (like UNFRIENDLY)?" Subjects in the SBDT condition were told their target behavior, and were instructed that a trait word would appear on the screen at each trial. They were to respond yes or no to indicate whether "<target behavior>-ing someone is that sort of action." Correspondingly, subjects in the DBST condition were told their target trait and were instructed that a different behavior would appear on the screen at each trial. They were to indicate whether the displayed behavior "is a <target trait> sort of action."

The screen display in all cases consisted of just a single word. The target traits used were intelligent, dominant, and friendly, chosen to represent major dimensions of social judgment. The target behaviors were educate, control, and love, chosen to be relatively prototypical of the three target traits. Targets were randomly assigned between subjects. The varying traits and behaviors were chosen without replacement from lists of 250 words of each type. Assignment of words to blocks and to trial positions within blocks was randomized separately for each subject.

Each subject made 250 judgments in this experiment, divided into five blocks of 50 trials each. Each trial within a block followed the response to the previous trial with a 750-msec delay. Between blocks, subjects were given a 1-2 min rest and were urged to stretch, get up from their chair and walk around, and generally relax their concentration. The speed and accuracy instructions, given at the beginning of the experiment, were repeated as a reminder before the second block. Before the fifth block subjects were given a new target trait or behavior, depending on their condition. For example, a subject who had been judging behaviors on the trait of intelligence for the first four blocks might be given friendly as the new target trait for the fifth block. Besides the changed target trait or behavior, the instructions and procedure remained the same for the fifth block.

Within each block, some words on the screen were repeated. Two items per block were repeated at each of seven different lags: 1, 2, 4, 6, 8, 12, and 16. Lag 1 means that the same word appeared on successive trials; lag 2 means that one unrelated word intervened between the

repetitions, and so on. Thus, each block of 50 trials included 14 words that the subject had encountered earlier in the block, as well as 36 new words (of which 22 were unique and 14 would later be repeated). The location of repetitions within block and the assignment of specific words to be repeated were randomized separately for each subject.

Results

The RT results were edited by treating all observations less than 400 msec as missing data (17 observations; these are ordinarily caused, with our equipment, by subjects inadvertently pressing the response key before the display for the trial is presented). All observations over 4500 msec (39 observations, 12 from one slow subject) were replaced by 4500, to lessen the influence of outliers on the analyses.

We analyzed the 6000 RT observations (24 subjects • 250 trials) with a regression model that takes account of subject differences, the effects of condition, specific and general practice effects, and the repetition within blocks. The model also includes a special term for the first trial of each block, because examination of plots (see Figure 4 below) revealed that they are substantially slower than other responses. General practice refers to the number of trials of the judgment process, ranging from 1 to 250 for each subject through the [illegible]. Specific practice refers to the number of trials with [illegible] target trait or behavior, and it ranges from 1 to 200 for the first 4 blocks and then from 1 to 50 for the fifth block, after the judgment target was changed. The effects of specific and general practice are assumed to be additive in the log-log model used to fit the power function of practice; this means that they are multiplicative in

the raw RT metric. This assumption is based on Anderson's ACT* model of practice effects, where the effects on RT of data strengthening (caused by practice on a specific target item) and procedural strengthening (caused by practice of the general process) are multiplicative (Anderson, 1983; Pirolli & Anderson, 1985).

This model was fit by OLS multiple regression with log(RT) as the dependent variable. The final model contained the following terms: conditions (dummy variables for 2 conditions), subjects within conditions (dummy variables for the 24 subjects), first trial of block (a dummy variable having the value 1 for the first trial of each block and 0 otherwise), specific practice (values from 1 to 200 for the first four blocks and then 1 to 50 for block 5, log-transformed), general practice (1 to 250, log-transformed), and item repetition (dummy variables for each of the seven different lags). Preliminary analyses tested and rejected as nonsignificant the interactions of condition with specific and general practice, with the first-trial dummy, and with repetition (all p's > .25). The fit of the final model was quite good, with an R^2 of 0.38 (MSE = .124 in log units, an experimental error of roughly 400 msec at the mean).

Table 2 about here.

The regression parameters and significance tests are presented in Table 2. The effect of condition (DBST versus SBDT) is nonsignificant; its MS is approximately the same as that for subjects, since condition is a between-subjects factor. As noted above, condition also did not

significantly interact with other terms in the model. Individual subjects, of course, differ greatly in their overall speed of responding.

The first trial of each block is estimated to be .38 log units (approximately 400 msec) slower than the average speed at that point in the task. This effect is presumably due to subjects readjusting to their task after the brief interval between blocks, and will not be further interpreted.

The effects of general and specific trials of practice are notable. The larger effect is for general practice, whose parameter is three times that for practice with the specific target item. These results imply that most of the benefits of practice on RT apply even to judgments with a new target; practice effects are *not* narrowly content-specific. This point will be discussed in more detail below.

The effects of item repetition are highly significant. Responses to repeated items are faster, much faster (-.45 log units or about 450 msec) if the repetition is immediate or if only one other item comes in between. But the amount of facilitation does not continue to decrease as lag increases. At all tested lags greater than 2 there is a roughly constant facilitation of responses to repeated items, even with 15 unrelated items intervening. This constant long-term effect is about 120 msec (-.11 log units evaluated at the mean). Figure 3 shows the amount of facilitation due to repetition plotted against lag. The standard error of each plotted value is .025.

Figure 3 about here.

Theoretical Model Fitting

To explore the data further, we fit a model with an asymptote term to the RTs. We thereby assume that only a portion of the initial RT can be eliminated by practice; there is some nonzero asymptotic time which would be reached by a large number of practice trials. (The regression model above assumes a zero asymptote.) The asymptote can be estimated in Experiment 2, because 250 trials of practice were used (not just 60 as in Experiment 1, where the estimation procedure described in this section failed to converge). The model used here is based on that of Pirolli and Anderson (1985). This specification is like the regression above in allowing for a slower first trial in each block and in assuming that general and target-specific practice have multiplicative effects on RT (as ACT* predicts). Specifically, the model is:

$$RT = a + b \cdot S^{e} \cdot G^{f} + c \cdot F$$

where a is the asymptote, b is the portion of the initial RT that can be reduced through practice, S is the number of trials of specific practice, G is trials of general practice, and F is the dummy variable representing the first trial of a block. The parameters e and f represent the effects of specific and general practice respectively, while c represents the size of the first-trial effect.

This model was estimated on the mean RTs for the 250 trials averaged across subjects. Besides general and specific practice and the first-trial effect, the only other significant effect on RT is for item repetition, and repetitions are randomly distributed across trials (except that the first k trials of a block can't involve repetitions with lag k or greater). Repetition effects therefore cannot bias these

results. Nonlinear estimation was by the Gauss-Newton method. The results show an asymptote of 490 msec (standard error 151 msec), the reducible time b equal to 1651 msec (112 msec), e equal to -.050 (0.013), f equal to -.169 (0.033), and c equal to 628 msec (39 msec). All parameters differ reliably from zero.[3] R^2 is 0.84, and the mean square error is 6805 msec (RMS error 82 msec). These results agree qualitatively with those of the zero-asymptote regression model: general practice is more important than specific. The parameters imply that the 250 trials reduce the RT 68% of the way to the asymptotic level (ignoring the first-trial effect--from 2141 to 1024 msec); another 534 msec or so could theoretically be eliminated by further practice.

Note that this is a model for the trial-by-trial means across subjects. Hence a high R^2 value (.84 compared to .38 for the model of the individual observations) is to be expected because random error and subject differences are averaged out to some extent in the means. The parameters from this analysis would not be expected to fit all individual subjects well, for subjects differ considerably in their average RT.

Figure 4 shows the trial-by-trial averages on which this analysis is based as well as the fitted function. The plotted function ignores the first-trial effect, which raises the five circled data points considerably above the general trend. Note that if all the practice effect was general, the RT would decrease monotonically across trials toward the asymptotic level (the dashed line) without the small jump shown at trial 201. However, the specific effect was significant, and in Figure 4 it can be seen that extending the general trend line through

trial 250 would underpredict the RTs for the final 50 trials. On the other hand, if all the effect was specific and all the speedup was due to practice with the specific target item, block 5 would look exactly like block 1, since it constitutes the first 50 trials of practice with a new target. Obviously this pattern bears no similarity to the data. The actual model estimates show that most of the total practice effect is due to general practice. Performance on block 5 is much better than on block 1 and only slightly worse than on block 4, because practice on the target item used in blocks 1-4 *greatly helps performance* on the different block 5 target.

Figure 4 about here.

Discussion

Experiment 2 produced several noteworthy results. First, the SBDT and DBST conditions do not differ in any detectable way. The difference observed in Experiment 1 was probably artifactual, most likely due to the different position of the trait and behavior words in the question on the screen. Instead, these results show the generality of the speedup function, as do those of Smith and Lerner (1986). The two different judgment tasks (focusing on a target trait versus on a behavior) *produced indistinguishable results.*

The effects of practice are mostly general, not target-specific, in agreement with the results of Pirolli and Anderson (1985). They used a fact-retrieval task where subjects practiced recalling facts they had learned in the experiment; the introduction of new facts in different

sessions made it possible to separate the effects of practice with the task in general from specific practice with each individual fact. They estimated that 30-50% of the speedup was due to general practice; the comparable figure in our experiment is 77% (in the model with an asymptote). Clearly, practice effects can be general, and the general effects can be even stronger than the target-specific ones.

The effects of item repetition in this experiment precisely parallel *those found* by Ratcliff, Hockley, and McKoon (1985) and Logan (1986) with the lexical decision task. This is a task *in which* subjects see letter strings on a screen and judge whether or not they are English words, and it is similar to our task in being a judgment based on long-term "semantic" knowledge structures. These researchers also repeated items at varying intervals and obtained results virtually identical to ours: a large short-term facilitation that lasts only a few seconds (across one intervening item at most), and a smaller but constant long-term facilitation that lasts up to the longest lag tested (lag eight for Ratcliff et al. and up to 24 for Logan). Facilitation from repetitions that is constant within a session has been observed by Salasoo, Shiffrin, and Feustel (1985) using a slightly different task, identification of perceptually degraded words.

Ratcliff et al. argue that the short-term effect is due to the activation *of nodes representing the items in memory (as assumed by* Anderson, 1983) and that the long-term effect is either due to the strengthening of item-specific procedures or to an episodic memory trace (pp. 448-9). Such an episodic trace might well involve not only a record of the identity of the previously encountered item but also of

the response that was given, as in Logan's (1986) instance theory. Evidence for this is the lack of any facilitation from an earlier encounter with an item if it was processed in a different way (Ratcliff et al., 1985, Experiment 2). Thus, a simple memory trace of the item alone may not produce facilitation; both the item and response may have to be repeated. The two hypotheses of strengthening an item-specific procedure for generating an appropriate response, and storing an item-response pair, will be very difficult to separate experimentally. As Logan's instance theory postulates, retrieval of the stored item-response pair can be used as a substitute for performing the process.

General Discussion

An increase in processing efficiency and hence in speed follows from practice with many different social and nonsocial cognitive tasks. What is its theoretical basis? The results of these experiments, particularly the observed pattern of generality of practice effects, help answer this question with respect to social judgments concerning behaviors and traits.

On the most specific side, some part of the speedup is specific to an item, occurring only when a stimulus item is repeated identically. This is most naturally accounted for by the instance theory of Logan (1986). People store information about previously encountered items and their associated responses, and can retrieve information from memory to respond quickly when the same item appears again (assuming the target and therefore the correct response has not changed). Applied to social judgments, this theory might imply that making a particular inference

(for example, judging that our friend George is creative) will be particularly efficient if the same inference about the same person has been made previously. Our study (and others cited above) show that facilitation from repetition lasts a surprisingly long time without any evident decay in strength, far longer than the duration of activation of the memory nodes involved. Research in other paradigms (e.g., Tulving, Schacter, & Stark, 1982) shows that facilitation induced by prior exposure to the identical item can last at least a week and can be independent of the *conscious* ability to remember the *prior exposure*. Thus, repetition effects constitute a form of implicit memory (Schacter, 1987). The fact that this trait judgment was made about George in the past may not be consciously retrievable, but because a repetition of the identical judgment will be facilitated, the prior judgment may nevertheless influence the perceiver's reactions on future occasions.

There is also a component of speedup that is target-specific but applies to new, nonrepeated stimulus items. Making a series of judgments about a fixed target may activate stored knowledge about the target *in memory* (i.e., a *schema*), making information relevant to the target more readily accessible. Both the theory of Shiffrin and Schneider (1977) and the data strengthening component of Anderson's (1983) theory predict such content-specific effects of practice. For social judgments, the implication is that one can become efficient in making a particular type of judgment (say concerning creativity) by making judgments on that dimension about many different people. This target-specific efficiency or expertise is the type assumed by Fong and Markus (1982) and other theorists within social cognition.

One unanswered question is the degree of <u>process</u> specificity of the target-specific skill. That is, will processing the same target concept with a different task benefit from practice? For example, will subjects who practice judging whether many behaviors imply a target trait gain efficiency at the task of judging whether many famous persons have that same trait? The answer to this question will show whether target specific speedup is actually due to data strengthening (in which case it should generalize across tasks as long as they access the same data structure in memory) or due to the strengthening of target-specific procedures. Smith and Branscombe (in press) present data suggesting that the latter is the case, using dependent measures drawn from the category accessibility paradigm of Srull and Wyer (1979), but further investigation is needed.

Despite the focus in the social cognition literature on target-specific expertise, the parameter estimates from Experiment 2 show that target-specific speedup is smaller in magnitude than a general speedup of the judgment process itself, which is applicable even to a different target. Of the theories reviewed, only Anderson's (1983) procedure strengthening mechanism directly predicts this finding. For social judgments, our findings imply that practice can improve general skills like making trait inferences from behaviors--the effects of practice need not be limited to the specific target trait that is practiced. Of course, the practiced trait will benefit somewhat more than unpracticed traits because it is influenced by both data and procedure strengthening (in Anderson's terms).

<u>Outstanding issues</u>

One question raised by our conclusion that practice effects are

not all target-specific comes from the failure to observe a significant speedup in the DBDT or the embedded SBDT or DBST conditions in Experiment 1. Judgments with one target seemed to require <u>consistent</u> practice, rather than practice intermixed with other targets, before the procedures were strengthened in a way that could support transfer to a new target. This issue deserves further theoretical and empirical consideration, but we have a few observations to make. First, some speedup was evident in the embedded conditions that focused on the same trait in Experiment 1, showing that procedures can benefit under some circumstances even when they are practiced intermixed with others. Also, Experiment 1 had low power with only 60 trials per subject; perhaps more extensive practice would demonstrate speedup in embedded conditions. Second, Smith and Lerner (1986) specifically manipulated the amount of practice that subjects received with one target before a transfer to a new target. They observed that a switch after just 5 trials was disruptive, preventing subjects from ever attaining a significant speedup. Only after 11 or more trials was the transfer relatively smooth, like that portrayed in Figure 4 of this paper.

On the theoretical level our suggestion, based on these findings, is that general procedures can be strengthened by practice <u>only after they have been compiled by a few trials of consistent practice</u>. In Anderson's model (1982), task-specific procedures are created as the task is performed initially in an "interpretive" (nonprocedural) mode. It may be that this compilation process is quite sensitive to the consistency of the initial trials of practice, and that the strengthening of procedures by practice can begin only after compilation

has occurred. The Smith and Lerner (1986) results and Salasoo et al.'s (1985) findings from a different paradigm converge on the suggestion that a half-dozen or so trials of a task with consistent contents are necessary for the formation of procedures. This suggestion carries the testable implication that speedup could be observed in embedded conditions if a small number of consistent trials came first.

The other question that may arise upon consideration of the results reported here is whether this paradigm may somehow lose the social nature of the judgments subjects make. Do subjects speed up simply by adopting narrow criteria for trait judgments, so that the judgments lose the complexity and open-endedness that are characteristic of social judgments in general? This general concern is of some import because, if valid, it casts a shadow over the results of most social cognition studies (at least those that use textual stimulus materials, like most of those cited in this paper). Obviously the concern is difficult to allay in a general sense, because that would require knowledge of the exact processes that subjects use to perform their tasks, and this knowledge is not yet in hand; discovering it is the long-term goal of social cognition research as a whole. However, four considerations lead us to discount this concern as a major problem affecting the results we report.

First, numerous studies show that experts differ from novices in various real-world domains such as medical diagnosis or chess playing (e.g., Chase & Simon, 1973) both in being more efficient in making decisions and in making better quality decisions (e.g., taking account of more rather than less of the relevant information). Clearly, speed

in making judgments that is attained through practice need not always come at the expense of a narrowed focus and less adequate decisions. Why should social judgments uniquely require such a trade-off? Second, there is no obvious way to make behavior-trait judgments like those used in these studies without bringing in large (theoretically unbounded) amounts of social knowledge. Let the reader judge whether the following behaviors (picked at random from the list of Experiment 2) are intelligent: leave, ask, fluster, consult, whip, sue, enjoy. Decisions about some behaviors (such as research or flunk) may be simple, requiring only access to semantic knowledge about word meanings. But for most behaviors one cannot judge trait implications without considering the nature of interpersonal relationships, social contexts, personality characteristics, and so on. No definitive answer can be provided by a limited body of information. For us, this open-ended quality is one defining characteristic of social decisions, and behavior-trait judgments clearly possess it.

Third, research by Fazio and his associates on attitude access (reviewed in Fazio, 1986) is conceptually similar to our studies in that subjects who practiced accessing their attitude (i.e., reported it repeatedly under experimental instructions) became faster at accessing it. Fazio went on to demonstrate that this manipulation actually increased attitude-behavior consistency when subjects were given an opportunity to act in an attitude-relevant way. If subjects speed up through practice only by adopting some artificial response criterion, one would not expect practice manipulations to have effects on actual behavior.

Finally, the examination of response consistency when items are repeated is relevant to this question. Unfortunately, the program used in Experiment 2 recorded response times but not the content of subjects' responses. However, in a recent experiment using a very similar method (Smith & Branscombe, unpublished) we have collected response consistency data. In four blocks of trait judgments, subjects (N=9) changed their response (from yes to no or vice versa) on 6.8% of the repeated behaviors. (This compares to a 4.0% error rate on a simple nonsocial judgment, telling whether a target letter appeared in the word, which can be taken as a baseline error rate of lapses in attention, slips of the finger, etc.) This rate of changed responses is low enough to convince us that subjects are not responding at random. It also exceeds the baseline rate by enough to show that the social quality of the judgment is reflected in some uncertainty and even changes in opinion on repeated questioning. Most important, the rate of response changes on trait judgments did not systematically change over blocks (chi-square(3) = 2.54, $p > .50$). The rates were 6.7%, 4.3%, 8.6%, and 7.4% on the four blocks, over which a substantial speedup was observed. This empirical consistency supports the conclusion we draw based on all of these considerations, that subjects speed up through practice by a true increase in efficiency, rather than by shifting their criteria and losing the social nature of the judgments.

Conclusions

In sum, no single existing theory is adequate to explain the patterns of changed performance induced by practice in these experiments

on social judgments. It appears that an integration of Anderson's (1983) procedural strengthening together with Logan's (1986) episodic storage of instances is required to account for both the general, non-target-specific speedup and the facilitation for repeated items. Current theoretical notions within social cognition, which emphasize changes in the accessibility of schemas or constructs, predict only target-specific patterns of speedup. The implications of practice and expertise for social judgment and social behavior, which have largely been conceptualized in terms of these prevailing theories, may therefore need to be rethought. Attention to the properties of cognitive procedures as well as cognitive contents (such as schemas) will be helpful in this task.

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Footnotes

1. Anderson's theory does include production composition and proceduralization mechanisms, which can restructure a process so that it takes fewer steps. However, this does not constitute a basic change in the way a process is performed in the sense that Logan's instance theory hypothesizes.

2. However, different versions of a conceptually similar instance model of speedup could predict a power-law relationship, as demonstrated by Logan (1986).

3. The asymptotic standard errors shown are approximate. Therefore the significance of the specific-practice parameter *e*, which is the only one whose significance is questionable, was checked by fixing that parameter at zero and re-estimating the model. The fit was significantly worse, with $F(1, 245) > 20$.

Table 1

Speedup Coefficients for Experimental and Dummy Trials

Experiment 1

Condition	Experimental Trials	Dummy Trials
Nonembedded		
Different B, Same T	-.215***	--
Same B, Different T	-.127***	--
Different B, Different T	-.002	--
Embedded		
Same B, Same T	-.39 ***	.024
Different B, Same T	-.029	.041
Same B, Different T	-.024	-.019
Different B, Different T	.025	.035

Note: ***p < .001.

Table 2

Regression Model for Trial-by-Trial RTs, Experiment 2

Source of Variation	MS	df	p
Between Subjects			
Condition	13.39	1	n.s.
Subject within Condition	11.57	22	--
Within Subjects			
General Practice	41.30	1	.0001
Specific Practice	3.74	1	.0001
Repetitions	9.12	7	.0001
First Trial of Block	16.23	1	.0001
Error	0.124	5949	--

Model Parameters

General Trials	-0.099	
Specific Trials	-0.027	
First Trial of Block	0.38	

Repetitions Lag	Effect	p
1	-0.45	.0001
2	-0.26	.0001
4	-0.11	.0001
6	-0.13	.0001
8	-0.11	.0001
12	-0.11	.0001
16	-0.10	.0001

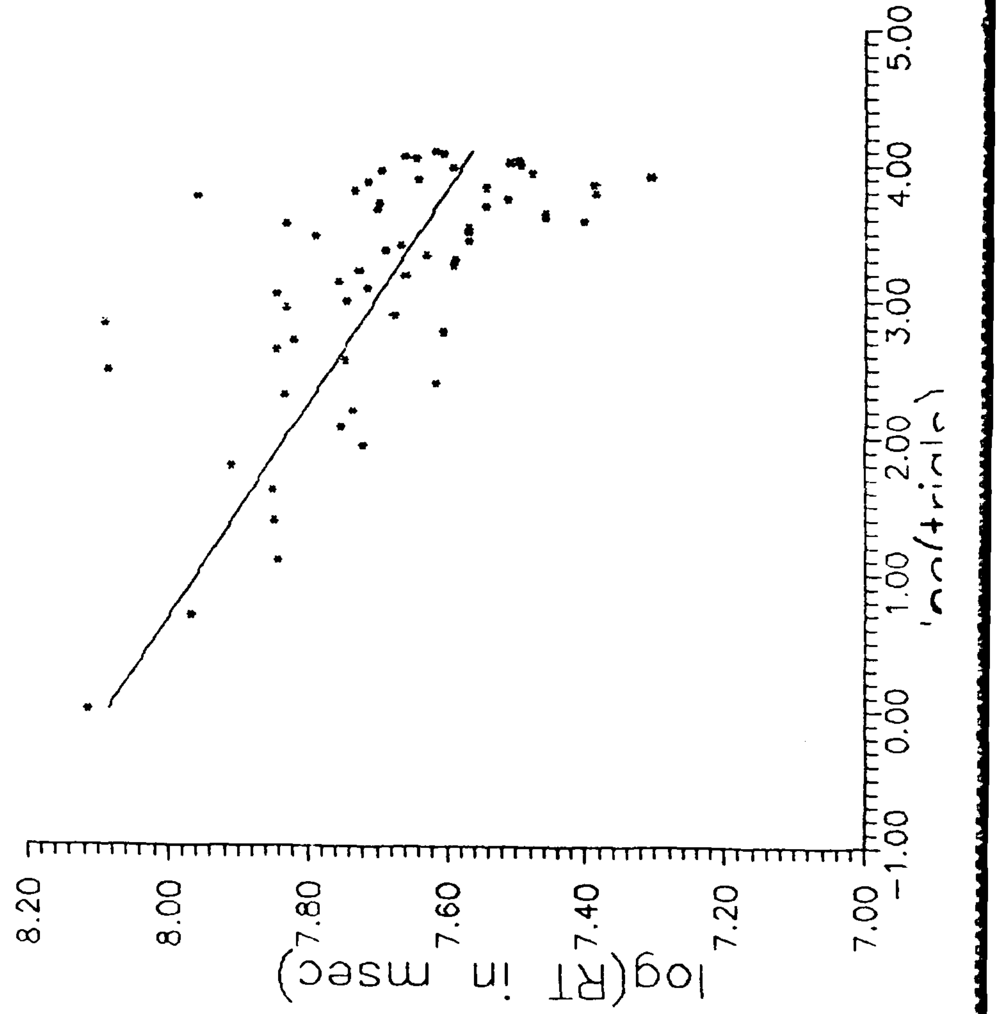
Figure Captions

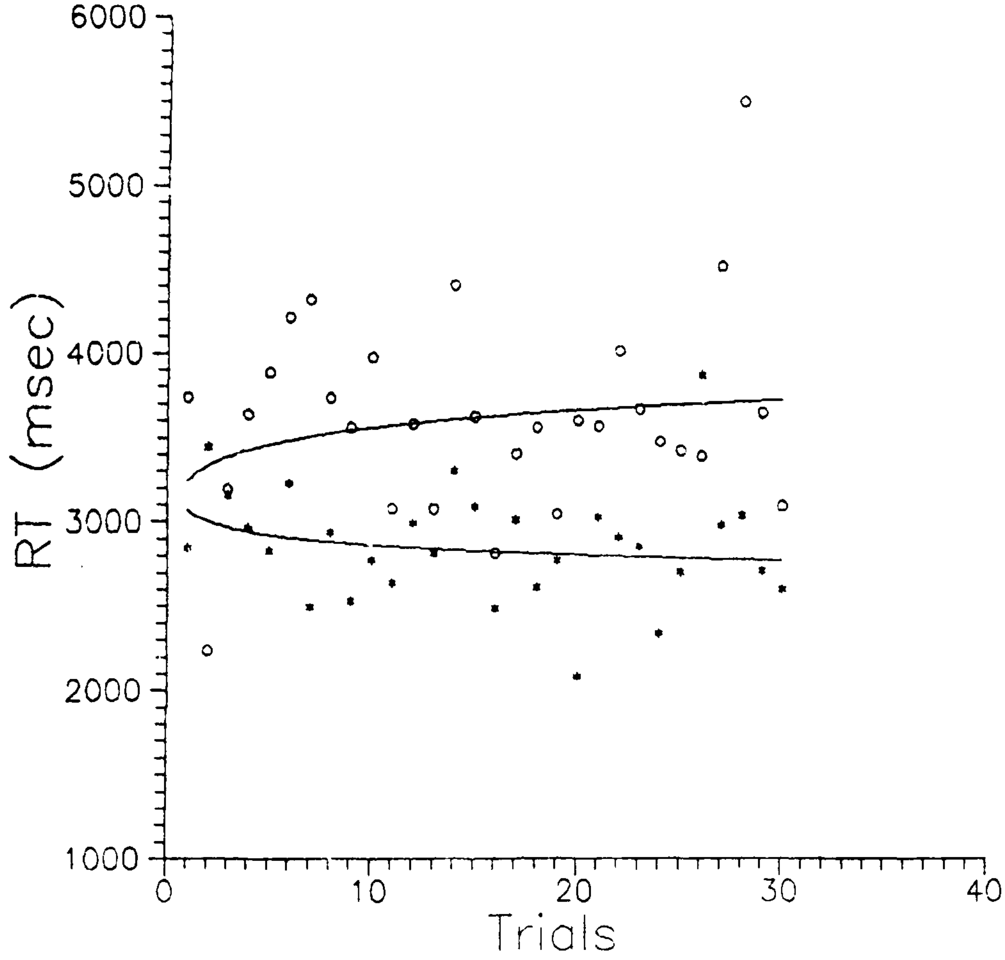
Fig. 1. Log-log plot of speedup in SBDT-nonembedded condition, Experiment 1. Plotted values are averages across 12 subjects.

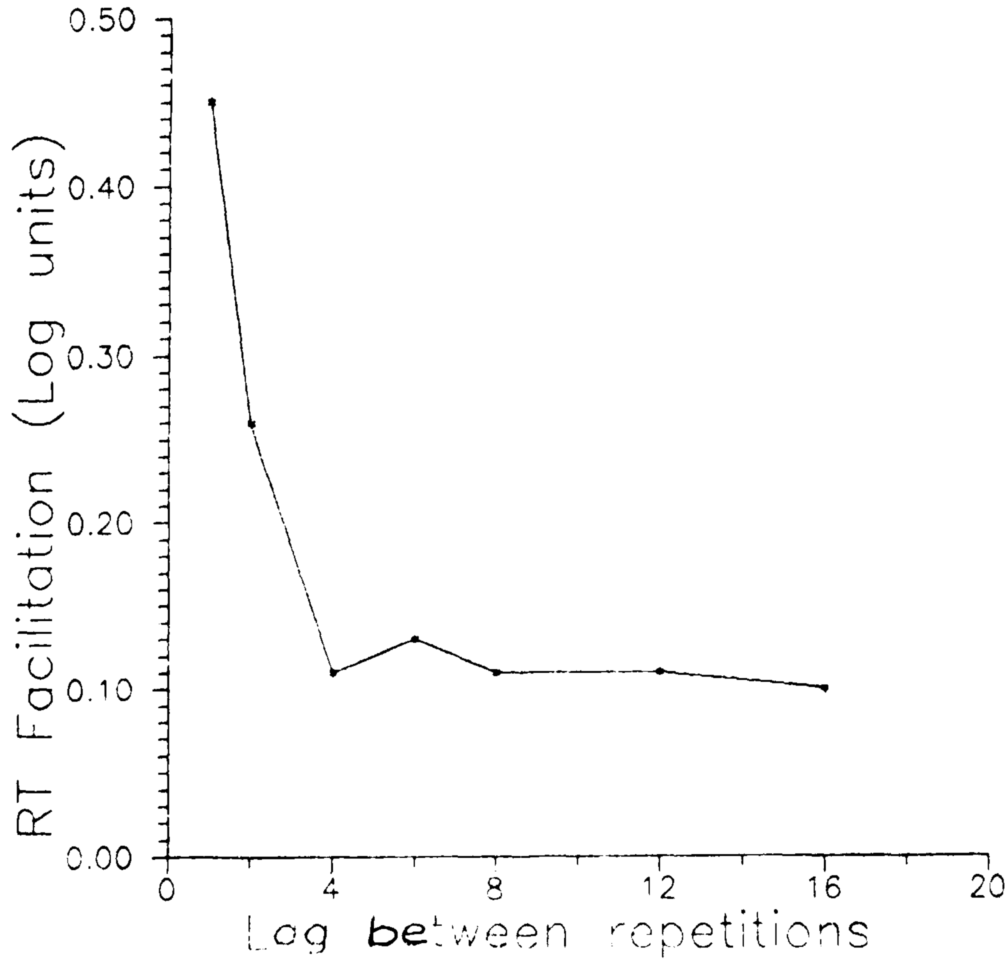
Fig. 2. DBST-embedded condition results, Experiment 1. Shown are separate power-function fits to dummy trials (open circles) and experimental trials (asterisks).

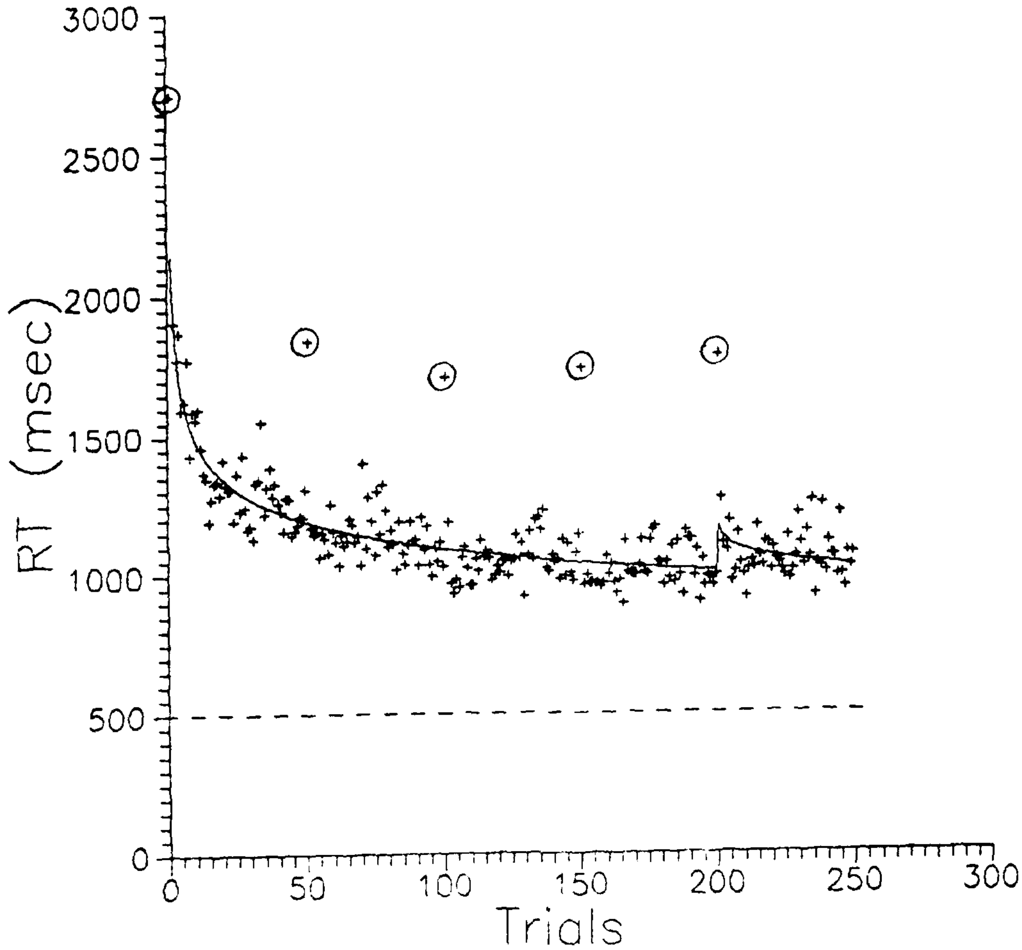
Fig. 3. Facilitation in RT from repetition by lag between repetitions, Experiment 2.

Fig. 4. Trial-by-trial average RT's across 24 subjects, Experiment 2. Shown is the theoretical model fit (ignoring the first-trial-of-block effect, which raises the five circled data points above the general trend).




6000
5000
4000
3000
2000
1000
RT (msec)
0
10
20
30
40
Trials


0.50
0.40
0.30
0.20
0.10
0.00
RT Facilitation (Log units)
0
4
8
12
16
20
Log between repetitions


3000
2500
2000
1500
1000
500
0
RT (msec)
0
50
100
150
200
250
300
Trials

END

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